XXV. *Thoughts on the conſtituent Parts of Water and of Dephlogiſticated Air; with an Account of ſome Experiments on that Subject. In a Letter from Mr.* James Watt, *Engineer, to Mr.* De Luc, *F. R. S.*

Read April 29, 1784.

DEAR SIR,

Birmingham,
November 26, 1784.

IN compliance with your deſire, I ſend you an account of the hypotheſis I have ventured to form on the probable cauſes of the production of water from the deflagration of a mixture of dephlogiſticated and inflammable airs, in ſome of our friend Dr. PRIESTLEY'S experiments.

I feel much reluctance to lay my thoughts on theſe ſubjects before the public in their preſent indigeſted ſtate, and without having been able to bring them to the teſt of ſuch experiments as would confirm or refute them; and ſhould, therefore, have delayed the publication of them until theſe experiments had been made, if you, Sir, and ſome other of my philoſophical friends, had not thought them as plauſible as any other conjectures which have been formed on the ſubject; and that though they ſhould not be verified by further experiments, or approved of by men of ſcience in general, they may perhaps merit a diſcuſſion, and give riſe to experiments which may throw light on ſo important a ſubject.

I firſt thought of this way of ſolving the phænomena in endeavouring to account for an experiment of Dr. PRIEST-

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LEY's, wherein water appeared to be converted into air; and I communicated my ſentiments in a letter addreſſed to him, dated April 26, 1783*, with a requeſt that he would do me the honour to lay them before the Royal Society; but as, before he had an opportunity of doing me that favour, he found, in the proſecution of his experiments, that the apparent converſion of water into air, by expoſing it to heat in porous earthen veſſels, was not a real tranſmutation, but an exchange of the elaſtic fluid for the liquid, in ſome manner not yet accounted for; therefore, as my theory was no ways applicable to the explaining theſe experiments, I thought proper to delay its publication, that I might examine the ſubject more deliberately, which my other avocations have prevented me from doing to this time.

1. It has been known for ſome time, that inflammable air contained much phlogiſton; and Dr. PRIESTLEY has found, by ſome experiments made lately, that it "is either wholly "pure phlogiſton, or at leaſt that it contains no apparent mix-"ture of any other matter." (In my opinion, however, it contains a ſmall quantity of water and much elementary heat.)

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* This letter Dr. PRIESTLEY received at London; and, after ſhewing it to ſeveral Members of the Royal Society, he delivered it to Sir JOSEPH BANKS, the Preſident, with a requeſt that it might be read at ſome of the public meetings of the Society; but before that could be complied with, the author, having heard of Dr. PRIESTLEY's new experiments, begged that the reading might be delayed. The letter, therefore, was reſerved until the 22d of April laſt; when, at the author's requeſt, it was read before the Society. It has been judged unneceſſary to print that letter, as the eſſential parts of it are repeated, almoſt *verbatim*, in this letter to M. DE LUC; but, to authenticate the date of the author's ideas, the parts of it which are contained in the preſent letter are marked with double commas.

heat*.) "He found, that by expoſing the calces of metals "to the ſolar rays, concentrated by a lens, in a veſſel containing inflammable air only, the calces of the ſofter metals "were reduced to their metallic ſtate;" and that the inflammable air was abſorbed in proportion as they became phlogiſticated; and, by continually ſupplying the veſſel with inflammable air, as it was abſorbed, he found, that out of 101 ounce meaſures, which he had put into the veſſel, 99 ounce meaſures were abſorbed by the calces, and only two ounce meaſures remained, which, upon examination, he found to be nearly of the ſame quality the whole quantity had been of before the experiment, and to be ſtill capable of deflagrating in conjunction with atmoſpheric or with dephlogiſticated air. *Therefore, as ſo great a quantity of inflammable air had been abſorbed by the metallic calces; the effect of reducing them to their metallic ſtate had been produced; and the ſmall remaining portion was ſtill unchanged, at leaſt had ſuffered no change which might not be attributed to its original want of purity; it was reaſonable to conclude, that inflammable air muſt be the pure phlogiſton, or the matter which reduced the calces to metals.*

2. "The ſame ingenious philoſopher mixed together certain proportions of pure dry dephlogiſticated air and of pure "dry inflammable air in a ſtrong glaſs veſſel, cloſely ſhut, "and then ſet them on fire by means of the electric ſpark," in the ſame manner as is done in the inflammable air piſtol. "The firſt effect was the appearance of red heat or inflamma-

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* Previous to Dr. PRIESTLEY's making theſe experiments, M. KIRWAN had proved, by very ingenious deductions from other facts, that inflammable air was, in all probability, the real phlogiſton, in an aerial form. Theſe arguments were perfectly convincing to me; but it ſeems more proper to reſt that part of the preſent hypotheſis on the direct experiment.

“ tion in the airs, which was ſoon followed by the glaſs veſſel “ becoming hot. The heat gradually pervaded the glaſs, and “ was diſſipated in the circumambient air, and as the glaſs “ grew cool, a miſt or viſible vapour appeared in it, which “ was condenſed on the glaſs in the form of moiſture or dew *. “ When the glaſs was cooled to the temperature of the atmo- “ ſphere, if the veſſel was opened with its mouth immerſed in “ water or mercury, ſo much of theſe liquids entered, as was “ ſufficient to fill the glaſs within about $\frac{1}{100}$dth part of its “ whole contents; and this ſmall reſiduum may ſafely be con- “ cluded to have been occaſioned by ſome impurity in one or “ both kinds of air. The moiſture adhering to the glaſs, after “ theſe deflagrations, being wiped off, or ſucked up, by a “ ſmall piece of ſponge paper, firſt carefully weighed, was “ found to be exactly, or very nearly, equal in weight to the “ airs employed.”

“ In ſome experiments, but not in all, a ſmall quantity of “ a ſooty-like matter was found adhering to the inſide of the “ glaſs,” the origin of which is not yet inveſtigated; but Dr. PRIESTLEY thinks, that it ariſes from ſome minute grains of the mercury that was uſed in order to fill the glaſs with the air, which being ſuper-phlogiſticated by the inflammable air, aſſumed that appearance; but, from whatever cauſe it proceeded, “ the whole quantity of ſooty-like matter was too “ ſmall to be an object of conſideration, particularly as it did “ not occur in all the experiments.”

I am obliged to your friendſhip for the account of the experiments which have been lately made at Paris on this ſubject,

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* I believe that Mr. CAVENDISH was the firſt who diſcovered that the combuſtion of dephlogiſticated and inflammable air produced moiſture on the ſides of the glaſs in which they were fired.

with large quantities of theſe two kinds of air, by which the eſſential point ſeems to be clearly proved, that the deflagration or union of dephlogiſticated and inflammable air, by means of ignition, produces a quantity of water equal in weight to the airs; and that the water, thus produced, appeared, by every teſt, to be pure water. As I am not furniſhed with any particulars of the manner of making the experiment, I can make no obſervations on it, only that, from the character you give me of the gentlemen who made it, there is no reaſon to doubt of its being made with all neceſſary precautions and accuracy, which was farther ſecured by the large quantities of the two airs conſumed.

3. "Let us now conſider what obviouſly happens in the "caſe of the deflagration of the inflammable and dephlogiſti-"cated air. Theſe two kinds of air unite with violence, they "become red-hot, and upon cooling totally diſappear. When "the veſſel is cooled, a quantity of water is found in it equal "to the weight of the air employed. This water is then the "only remaining product of the proceſs, and *water*, *light*, and "*heat*, are all the products," unleſs there be ſome other matter ſet free which eſcapes our ſenſes.

"*Are we not then authoriſed to conclude, that water is com-"poſed of dephlogiſticated air and phlogiſton, deprived of part of "their latent or elementary heat*; *that dephlogiſticated or pure air "is compoſed of water deprived of its phlogiſton, and united to "elementary heat and light*; *and that the latter are contained in "it in a latent ſtate, ſo as not to be ſenſible to the thermometer or "to the eye*; *and if light be only a modification of heat, or a cir-"cumſtance attending it, or a component part of the inflammable "air, then pure or dephlogiſticated air is compoſed of water de-"prived of its phlogiſton and united to elementary heat?*"

4. "It appears, that dephlogiſticated water," or, which may be a better name for the baſis of water and air, the element you call *humor*, "has a more powerful attraction for "phlogiſton than it has for latent heat, but that it cannot "unite with it, at leaſt not to the point of ſaturation, or to "the total expulſion of the heat, unleſs it be firſt made red-"hot," or nearly ſo. "The electric ſpark heats a portion of "it red-hot, the attraction between the humor and the phlo-"giſton takes place, and the heat which is let looſe from this "firſt portion heats a ſecond, which operates in a like manner "on the adjoining particles, and ſo continually until the whole "is heated red-hot and decompoſed." Why this attraction does not take place to the ſame degree in the common temperature of the atmoſphere, is a queſtion I am not yet able to ſolve; but it appears, that, in ſome circumſtances, "dephlo-"giſticated air can unite, in certain degrees, with phlogiſton "without being changed into water." Thus Dr. PRIESTLEY has found, that by taking clean filings of iron, which, alone, produce only inflammable air of the pureſt kind, and *mercurius calcinatus per ſe*, which gives only the pureſt dephlogiſticated air, and expoſing them to heat, in the ſame veſſel, he obtained neither dephlogiſticated nor inflammable air, "but in their "place fixed air." Yet it is well known, that a mixture of dephlogiſticated and inflammable air will remain for years in cloſe veſſels in the common heat of the atmoſphere, without ſuffering any change, the mixture being as capable of deflagration at the end of that time as it was when firſt ſhut up. Theſe facts the Doctor accounts for, by ſuppoſing that the two kinds of air, when formed at the ſame time in the ſame veſſel, can unite in their *naſcent* ſtate; but that, when fully formed, they are incapable of acting upon one another, unleſs they are

firſt ſet in motion by external heat. "Phlogiſticated air ſeems "alſo to be another compoſition of phlogiſton and dephlogiſti-"cated air;" but in what proportions they are united, or by what means, is ſtill unknown. It appears to me to be very probable, that fixed air contains a greater quantity of phlogiſton than phlogiſticated air does, becauſe it has a greater ſpecific gravity, and becauſe it has more affinity with water.

5. "For many years I have entertained an opinion, that air "was a modification of water, which was originally founded "on the facts that in moſt caſes, wherein air was actually "made," which ſhould be diſtinguiſhed from thoſe wherein it is only extricated from ſubſtances containing it in their pores, or otherwiſe united to them in the ſtate of air, "the ſub-"ſtances were ſuch as were known to contain water as one of "their conſtituent parts, yet no water was obtained in the "proceſſes," except what was known to be only looſely connected with them, ſuch as the water of the cryſtallization of ſalts. "This opinion aroſe from a diſcovery," that the latent heat contained in ſteam diminiſhed in proportion as the ſenſible heat of the water from which it was produced increaſed; or, in other words, "that the latent heat of ſteam was leſs "when it was produced under a greater preſſure, or in a more "denſe ſtate, and greater when it was produced under a leſs "preſſure, or in a leſs denſe ſtate; which led me to conclude, "that when a very great degree of heat was neceſſary for the "production of the ſteam, the latent heat would be wholly "changed into ſenſible heat; and that, in ſuch caſes, the "ſteam itſelf might ſuffer ſome remarkable change. I now "abandon this opinion in ſo far as relates to the change of "water into air, as I think that may be accounted for on better "principles."

6. "In every caſe, wherein dephlogiſticated air has been "produced, ſubſtances have been employed, ſome of whoſe "conſtituent parts have a ſtrong attraction for phlogiſton, and, "as it would appear, a ſtronger attraction for that ſubſtance "than *humor* has; they ſhould, therefore, dephlogiſticate the "water" or fixed air, and the *humor* thus ſet free ſhould unite to the matter of fire and light and become pure air. Dephlogiſticated air is produced in great abundance from melted nitre. "The acid of nitre has a greater attraction for phlogiſton than "any other ſubſtance is known to have; and it is alſo certain, "that nitre, beſides its water of cryſtallization, contains a "quantity of water as one of its elementary parts, which "water adheres to the other parts of the nitre with a force "ſufficient to enable it to ſuſtain a red heat. When the nitre "is melted, or made red-hot, the acid acts upon the water and "dephlogiſticates it; and the fire ſupplies the *humor* with the "due quantity of heat to conſtitute it air, under which form "it immediately iſſues. It is not eaſy to tell what becomes of "the acid of nitre and phlogiſton, which are ſuppoſed to be "united," as they ſeem to be loſt in the proceſs. Dr. PRIESTLEY has lately made ſome experiments, with a view to aſcertain this point. He diſtilled dephlogiſticated air from pure nitre, in an earthen retort glazed within and without. He employed 2 oz $=$960 grains of nitre: the retort was placed in an air furnace, and, by means of an intenſe heat, he obtained from the nitre in one experiment 787, and in another experiment 800 ounce meaſures of dephlogiſticated air; and he found that, upon weighing the retort and nitre before and after the proceſs, they had ſuffered a loſs of weight equal to the weight of the air, and to the water of cryſtallization of the nitre, but nothing more. He remarked, that the air had a pungent

ſmell, which he could not diveſt it of by waſhing; and that the water in which the air was received had become ſlightly acid. I examined a portion of this water, which he was ſo kind as to ſend me, and found by it that the whole of the receiving water had contained the acid belonging to 2 drams = 120 grains of nitre. I alſo examined the reſiduum and the retort in which the diſtillation had been performed, and found the reſiduum highly alkaline, yet containing a minute quantity of phlogiſticated nitrous acid. It had acted conſiderably upon the retort, and had diſſolved a part of it, which was depoſited in the form of a browniſh powder, when the ſaline part was diſſolved in water. This earthy powder I have not yet thoroughly examined, but have no doubt that it principally conſiſts of the earth of the retort. This experiment, and all others tried in earthen veſſels, leave us ſtill at a loſs to determine what becomes of the acid and phlogiſton. They ſeem either to remain mixed with the air, in the form of an incoercible gas; or to unite with the alkali, or with the earth of the retort, in ſome manner ſo as not to be eaſily ſeparated from them; or elſe they are imbibed by the retorts themſelves, which are ſufficiently porous to admit of ſuch a ſuppoſition.

All that appears to be concluſive from this experiment is, that above one half of the weight of the nitre was obtained in the form of dephlogiſticated air; and that the reſiduum ſtill contained ſome nitrous acid united to phlogiſton.

7. Finding that the action of the nitre on the retort tended to prevent any accurate examination of the products, I had recourſe to combinations of the nitrous acid with earths from which the dephlogiſticated air is obtained with leſs heat than from nitre itſelf. As theſe proceſſes have been particularly deſcribed by Dr. PRIESTLEY, by Mr. SCHEELE, and others, I

ſhall not enter into any detail of them; but ſhall mention the general phænomena which I obſerved, and which relate to the preſent ſubject.

The earths I uſed were magneſia alba, calcareous earth, and minium or the red calx of lead. I diſſolved them in the reſpective experiments in nitrous acid dephlogiſticated by boiling, and diluted with proper proportions of water. I made uſe of glaſs retorts, coated with clay; and I received the air in glaſs veſſels, whoſe mouths were immerſed in a glazed earthen baſon, containing the ſmalleſt quantity of water that could be uſed for the purpoſe. As ſoon as the retort was heated a little above the heat of boiling water, the ſolutions began to diſtil watery vapours containing nitrous acid. Soon after theſe vapours ceaſed, yellow fumes, and in ſome of the caſes dark red fumes, began to appear in the neck of the retort; and at the ſame time there was a production of dephlogiſticated air, which was greater in quantity from ſome of theſe mixtures than from others, but continued in all of them until the ſubſtances were reduced to dryneſs. I found, in the receiving water &c. very nearly the whole of the nitrous acid uſed for their ſolution, but highly phlogiſticated, ſo as to emit nitrous air by the application of heat; and there is reaſon to believe, that with more precaution the whole might have been obtained.

8. As the quantity of dephlogiſticated air produced by theſe proceſſes did not form a ſufficient part of the whole weight, to enable me to judge whether any of the real acid entered into the compoſition of the air obtained, I ceaſed to purſue them further, having learned from them the fact, *that however much the acid and the earths were dephlogiſticated before the ſolution, the acid always became highly phlogiſticated in the proceſs.*

In order to examine whether this phlogiſton was furniſhed by the earths, ſome dephlogiſticated nitrous acid was diſtilled from minium till no more acid or air came over. More of the ſame acid was added to the minium as ſoon as it was cold, and the diſtillation repeated, which produced the ſame appearance of red fumes and dephlogiſticated air. This operation was repeated a third time on the ſame minium, without any ſenſible variation in the phænomena. The proceſs ſhould have been ſtill farther repeated, but the retort broke about the end of the third diſtillation. The quantity of minium uſed was 120 grains, and the quantity of nitrous acid added each time was 240 grains, of ſuch ſtrength that it could diſſolve half its weight of mercury, by means of heat.

It appears from this experiment, that unleſs minium be ſuppoſed to conſiſt principally of phlogiſton, the ſource of the phlogiſton, thus obtained, was either the nitrous acid itſelf, or the water with which it was diluted; or elſe that it came through the retort with the light, for the retort was in this caſe red-hot before any air was produced; yet this latter concluſion does not appear very ſatisfactory, when it is conſidered, that in the proceſs wherein the earth made uſe of was magneſia, the retort was not red-hot, or very obſcurely ſo, in any part of the proceſs; and by no means luminous, when the yellow and red fumes firſt made their appearance.

9. As the principal point in view was to determine whether any part of the acid entered into the compoſition of the air, I reſolved to employ ſome ſubſtance which would part with the acid in a moderate heat, and alſo give larger quantities of air than had been obtained in the former proceſſes. Mercury was thought a proper ſubſtance for this purpoſe. 240 grains of mercury were put into a glaſs retort with 480 grains

of diluted dephlogiſticated nitrous acid, which was the quantity neceſſary to diſſolve the whole of the mercury, a gentle heat was applied, and as ſoon as the common air contained in the retort was diſſipated, a veſſel was placed to receive the nitrous air proceeding from the ſolution, which was 16 ounce meaſures. When it had ceaſed to give nitrous air, the neck of the retort became hot from the watery ſteams of the acid. The air receiver was taken away, and a common receiver was luted on, with a little water in it, to condenſe the vapours, and a quantity of dilute, but highly phlogiſticated, acid was caught in the receiver. When the watery vapours had nearly come over, and yellow fumes appeared in the neck of the retort, the common receiver was removed, and the air receiver replaced; about four ounces of very ſtrong nitrous air paſſed up immediately, the fumes in the retort became red, and dephlogiſticated air paſſed up, which, uniting with the nitrous air in the receiver, produced red fumes in the receiver; and the two kinds of air acting upon one another, their bulk was reduced to half of an ounce meaſure. At this period the fumes in the retort were of a dark red colour, and dephlogiſticated air was produced very faſt. After a ſhort time, ſome orange-coloured ſublimate appeared in the upper part of the retort, and extended a little way along its neck, the red colour of the fumes gradually diſappeared, and the neck of the retort became quite clear. At the ſame time that this happened, ſmall globules of mercury appeared in the neck of the retort, and accumulated there until they ran down in drops. The production of the air was now very rapid, and accompanied with much of the white cloud or powdery matter, which paſſed up with the air into the receiver, and mixed with the water, but did not diſſolve in it. After giving about 36 ounce meaſures of dephlogiſticated air, it ſuddenly ceaſed to give any more; and the retort being cooled, the bulb was found to be quite empty, excepting a ſmall quantity of black powder, which, on being rubbed on the hand, proved to be moſtly running mercury. The orange-coloured ſublimate was waſhed out of the neck of the retort, and what running mercury was in it was ſeparated, and added to that which had run down into the baſon among the water. The whole fluid mercury, when dried, weighed 218 grains; therefore 22 grains remained in the form of ſublimate, which, I believe, would alſo have been reduced if I could have applied heat in a proper manner to the neck of the retort, as ſome of it, to which heat could be applied, diſappeared.

10. The 16 ounce meaſures of nitrous air, which had been produced in the ſolution of the mercury, and had remained confined by water in the receiver, was converted into nitrous acid by the gradual admiſſion of common air, and was taken up by the water; this water was added to that in the baſon, which had ſerved to receive the dephlogiſticated air. The whole quantity was about two quarts, was very acid to the taſte, and ſparkling with nitrous air. It was immediately put into bottles, and well corked, until it had loſt the heat gained in the operation. In order to determine the quantity of acid in the receiving water and in the ſublimate, I diſſolved, firſt, alkali of tartar in water, and filtered the ſolution. 352 grains of this alkaline ſolution ſaturated 120 grains of the nitrous acid I had employed to diſſolve the mercury, and 1395 grains of the ſame alkaline ſolution ſaturated the orange-coloured precipitate, and all the acid liquor obtained from the proceſs: therefore we have the proportion as 352 : 120 :: 1395 : 475, from which it appears, that all the acid employed was recovered again in the form of acid, excepting only five grains;

a ſmaller quantity than what might reaſonably be ſuppoſed to be loſt in the proceſs by the extreme volatility of the nitrous air. In order to aſcertain the exact point of ſaturation, ſlips of paper, ſtained by the juice of the petals of the ſcarlet roſe, were employed, which were the niceſt teſt I could procure, as litmus will not ſhew the point of ſaturation of any liquor containing much phlogiſticated nitrous acid, or even fixed air, but will turn red, and ſhew it to be acid, when the teſt of thoſe leaves, violets, and ſome other of the like kind, will turn green in the ſame liquor, and ſhew it to be alkaline. But the exact point of ſaturation of ſo dilute a liquor is ſo very difficult to aſcertain, than an error might eaſily be committed, notwithſtanding the attention beſtowed upon it. Suppoſing this experiment to be unexceptionable, the concluſions which may be drawn from it are very favourable to the hypotheſis I endeavour to ſupport. *Thirty-ſix ounce meaſures of dephlogiſticated air were obtained, and only five grains of a weak nitrous acid were loſt in the proceſs. Two hundred and eighteen grains of mercury out of two hundred and forty were revived, and all the dephlogiſticated nitrous acid employed is found to be highly phlogiſticated in the proceſs. It appears, that the nitrous acid does not enter into the compoſition of dephlogiſticated air; it ſeems only to ſerve to abſorb phlogiſton from the watery part of the mercurial nitre.*

11. As this laſt proceſs proved very tedious and complicated on account of the neceſſity of aſcertaining the quantity of acid in the receiving water, by means of an alkali which afforded a double ſource of error in the point of ſaturation, I reſolved to try the diſtillation of dephlogiſticated air from cubic nitre in a glaſs veſſel, and to draw from it only ſuch a quantity of air as it would yield without acting much upon the retort, which

latter circumſtance is eſſentially neceſſary to be attended to. An ounce of the cryſtals of mineral alkali were diſſolved in nitrous acid, and the mixture brought to an exact ſaturation by the teſt of litmus; 30 ounce meaſures of air were diſtilled from it, which, during the latter part of the proceſs, was accompanied with ſlightly yellow fumes; the receiving water was found to be acid, and the reſiduum alkaline. The reſiduum being diſſolved in the receiving water, the ſolution was neutral, or very nearly ſo, by every teſt; for in this caſe litmus might be uſed, as the acid was very ſlightly phlogiſticated. On adding a few drops of a very dilute nitrous acid, the teſts ſhewed the liquor to be acid.

12. Encouraged by the ſucceſs of this experiment, I took an ounce = 480 grains of pure common nitre, and put it into a flint-glaſs retort, coated, which was placed in a furnace. It began to give air about the time it became red-hot, and during the latter part of the proceſs this air was accompanied with yellowiſh fumes. I ſtopped the proceſs when it had produced 50 ounce meaſures of air. The receiving water, and particularly the air, had a ſtrong but peculiar ſmell of nitrous acid. The air was well waſhed with the receiving water, but was not freed from the ſmell. The receiving water, which was 50 ounces, was ſlightly acid, and the reſiduum alkaline. I diſſolved the latter in the former, and found the mixture alkaline. 10 grains of weak nitrous acid were added to it, which ſaturated it, and 105 grains of this ſpirit of nitre was found to contain the acid of 60 grains of nitre; therefore the 10 grains contained the acid of 5,7 grains of nitre, which, by Mr. KIRWAN's experiments is equal to two grains of real nitrous acid. *We have, therefore, 34 grains weight of dephlogiſticated air produced, and only two grains of real acid miſſing*; and it is not

certain that this quantity was deſtroyed, becauſe ſome portion of the glaſs of the retort was diſſolved by the nitre, and ſome part of the materials employed in making the glaſs being alkali, we may conclude, that the alkali of the nitre would be augmented by the alkali of that part of the glaſs it had diſſolved. As the glaſs cracked into ſmall pieces on cooling, and ſome part of the coating adhered firmly to it, the quantity of the glaſs that was diſſolved could not be aſcertained. *From this experiment it appears, that if any of the acid of the nitre enters into the compoſition of the dephlogiſticated air, it is a very ſmall part; and it rather ſeems, that the acid, or part of it, unites itſelf ſo firmly to the phlogiſton as to loſe its attraction for water.*

13. "The vitriolic ſalts alſo yield dephlogiſticated air by "heat; and in theſe caſes the dephlogiſticated air is always "attended with a large quantity of vitriolic acid air or ſul"phureous vapour," even when the ſalts uſed are not known to contain any phlogiſtic matter. Mr. **Scheele** mentions his having obtained dephlogiſticated air from mangneſe diſſolved in acid of phoſphorus, and alſo from the arſenical acid: from whence it appears, that theſe acids, or perhaps any acid which can bear a red heat, can concur to the production of dephlogiſticated air. *It is neceſſary to remark, that no experiments have been yet publiſhed ſhewing that dephlogiſticated air can be produced from ſalts formed by the muriatic acid. The acids which produce ſalts ſuitable for this purpoſe, have all a ſtrong affinity with phlogiſton; and the marine acid has either a very ſmall affinity with it, or elſe is already ſaturated with it, at leaſt ſo far ſaturated as not to be able to attract it from the* humor.

14. "The dephlogiſticated air obtained from the pure calces "of metals may be attributed to the calces themſelves, attract"ing the phlogiſton from water which they have imbibed from

“ the atmoſphere, or from dephlogiſticating the fixed air which “ they are known to contain.”

It is very probable, that the dephlogiſticated air extruded from growing vegetables may be owing to their dephlogiſticating the water they grow in; but it appears more probable, that the plants have a power of dephlogiſticating the fixed, or phlogiſticated, air of the atmoſphere.

“ When dephlogiſticated and nitrous air are mixed, the de“ phlogiſticated air ſeizes part of the phlogiſton of the nitrous “ air.” The water contained in the nitrous air, and the other part of the phlogiſton, unite with the nitrous acid, which then aſſumes a liquid form, or at leaſt that of a denſe vapour; “ and “ that part of the latent heat of the two airs not eſſential to the “ new combination is ſet at liberty *.”

In the combuſtion of ſulphur the ſame thing happens, but in a greater degree; for the vitriolic acid, having a much weaker attraction for phlogiſton than air has, abandons it almoſt entirely to the latter, which is thereby converted into water, and in that form attracts the vitriolic acid, and reduces it to a liquid ſtate. The ſame reaſoning may be applied to the combuſtion of phoſphorus, which is attended with ſimilar effects.

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* I cannot take upon me to determine, from any facts which have come to my knowledge, whether any part of the dephlogiſticated air employed in this experiment is turned into fixed air; but I am rather inclined to think that ſome part is, becauſe the quantity of heat, which is ſeparated by the union of the two airs, does not ſeem to be ſo great as that which is ſeparated when the dephlogiſticated air is wholly changed into water: yet ſome water appears to be formed, becauſe when the mixture is made over mercury, the ſolution of the mercury in the nitrous acid aſſumes a cryſtallized form, which, however, may be due to the watery part of the nitrous air.

15. I ſhall not make, at preſent, any further deductions from what I myſelf conſider ſtill in the light of a conjectural hypotheſis, which I have perhaps dwelt upon too long already. I ſhall only beg your attention to ſome general reaſoning on the ſubject; which, however, may poſſibly ſerve more to ſhew the uncertainty of other ſyſtems on the conſtituent parts of air, than the certainty of this. Some of thoſe ſyſtems ſuppoſe dephlogiſticated air to be compoſed of an acid and ſomething elſe, ſome ſay phlogiſton. If an acid enters into the compoſition of it, why does not that acid appear again when the air is decompoſed, by means of inflammable air and heat? And why is the water which is the product of this proceſs pure water? And if an acid forms one of its conſtituent parts, why has nobody been able to detect any difference in the dephlogiſticated air, made by the help of different acids, when compared with one another, or with the air extruded by vegetables? Theſe airs, of ſuch different origins, appear to be exactly the ſame. And if phlogiſton be a conſtituent part of air, why does it attract phlogiſton with ſuch avidity? Some have, on the other hand, contended that air is compoſed of earth, united to acids or phlogiſton, or to both, or to ſome other matter. Here we muſt aſk, what earth it is which is one of the component parts of air? All earths which will unite with the nitrous or vitriolic acids, and with ſome others, ſuch as the phoſphoric and the arſenical acids, will ſerve as baſes for the formation of air, and the air produced from all of them appears by every teſt to be the ſame, when freed from accidental impurities. To this argument it is anſwered, that it is not any particular ſpecies of earth which is the baſis of air, but elementary or ſimple earth, which is contained in all of them. If this were the

matter of fact, would not that earth be found after the decompoſition of the air?

Mr. Scheele has formed an hypotheſis on this ſubject, in which he ſuppoſes heat to be compoſed of dephlogiſticated air united to phlogiſton, and that this combination is ſufficiently ſubtile to paſs through glaſs veſſels. He affirms, that the nitrous and other acids, when in an ignited ſtate, attract the phlogiſton from the heat, and ſet the dephlogiſticated air at liberty; but he does not ſeem to have been more ſucceſsful than myſelf in explaining what becomes of the acid of nitre and phlogiſton in the caſe of the decompoſition of nitre by heat. And ſince we know, from the late experiments, that water is a compoſition of air, or more properly, *humor* and phlogiſton, his whole theory muſt fall to the ground, unleſs that fact be otherwiſe accounted for, which it does not ſeem eaſy to do.

16. To return to the experiment of the deflagration of dephlogiſticated and inflammable air, "it appears from the "two airs becoming red-hot on their union, that the quantity "of heat contained in one or both of them, is much greater "than that contained in ſteam; becauſe, for the firſt moments "after the exploſion, the water depoſited by the air remains in "the form of ſteam, and conſequently retains the latent heat "due to that modification of water. This matter may be eaſily "examined by firing the mixture of dephlogiſticated and inflammable air in a veſſel immerſed in another veſſel containing a "given quantity of water of a known heat, and after the veſſel "in which the deflagration is performed is come to the ſame "temperature with the water in which it is immerſed, by examining how much heat that water has gained, which being "divided by the quantity of water produced by the decompoſition of the airs, will give the whole quantity of elemen-

“ tary or latent heat which that water had contained, both as “ air and as ſteam; and if from that quantity we deduct the “ latent heat of the ſteam, the remainder will be the latent or “ elementary heat contained more in air than in ſteam.” This experiment may be made more compleatly by means of the excellent apparatus which Meſſ. Lavoiſier and De la Place have contrived for ſimilar purpoſes.

Until direct experiments are made, we may conclude, from thoſe which have been made by the gentlemen juſt named, on the decompoſitions of air by burning phoſphorus and charcoal, that the heat extricated during the combuſtion of inflammable and dephlogiſticated air is much greater than it appears to be; for they found that one Paris ounce (= 576 Pariſian grains) of dephlogiſticated air, when decompoſed by burning phoſphorus, melted 68,634 ounces of ice; and as, according to another of their experiments, ice, upon being melted, abſorbs 135° of heat, by Fahrenheit's ſcale, each ounce of air gave out $68{,}634 \times 135^\circ = 9265^\circ{,}590$; that is to ſay, a quantity of heat which would have heated an ounce of water, or any other matter which has the ſame capacity for receiving heat as water has, from 32° to 9265½°: a ſurpriſing quantity! (It is to be underſtood, that all the latent heats mentioned herein are compared with the capacity of water). And when an ounce of dephlogiſticated air was changed into fixed air, by burning charcoal, or by the breathing of animals, it melted 29,547 oz. of ice; conſequently we have $29{,}547 \times 135^\circ = 3988^\circ{,}845$. the quantity of heat which an ounce of dephlogiſticated air loſes when it is changed into fixed air. By the heat extricated during the detonation of one ounce of nitre with one ounce of ſulphur, 32 ounces of ice were melted; and, by the experiment I have mentioned of Dr. Prieſtley's (6), it appears that nitre can produce one half of its weight of dephlogiſticated air. When the nitre and ſulphur are kindled, the dephlogiſticated air of the nitre unites with the phlogiſton of the ſulphur, and ſets its acid free, which immediately unites to the alkali of the nitre, and produces vitriolated tartar. The dephlogiſticated air, united to the phlogiſton, is turned into water, part of which is abſorbed by the vitriolated tartar, and part is diſſipated in the form of vapours, or unites to the nitrous air, or other air, produced in the proceſs.

As half an ounce of dephlogiſticated air is, in this proceſs, united by inflammation to a quantity of phlogiſton ſufficient to ſaturate it, and no fixed air is produced, it ſhould melt a quantity of ice equal to the half of what was melted by the combination of an ounce of air with phlogiſton in burning phoſphorus; that is, it ſhould melt 34,317 ounces of ice; and we find, by Meſſ. LAVOISIER and DE LA PLACE's experiment, that it actually melted 32 ounces of ice: the ſmall difference may be accounted for by ſuppoſing, that the heat produced by the combuſtion might not be quite ſo great as that Dr. PRIESTLEY employed in his experiment; or that the nitre might be leſs pure, and conſequently not ſo much air formed. The two facts, however, agree near enough to permit us to conclude, *that dephlogiſticated air, in uniting to the phlogiſton of ſulphur, produces as much heat as it does in uniting with the phlogiſton of phoſphorus.*

17. According to Dr. PRIESTLEY's experiments, dephlogiſticated air unites compleatly with about twice its bulk of the inflammable air from metals. The inflammable air being ſuppoſed to be wholly phlogiſton, and being $\frac{1}{9,6}$ of the weight of an equal bulk of dephlogiſticated air, and being double in quantity, will be $\frac{1}{4,8}$ of the weight of the dephlogiſticated air

it unites with. Therefore one ounce (576 grains) of dephlogiſticated air, will require 120 grains of inflammable air, or phlogiſton, to convert it into water. And ſuppoſing the heat extricated by the union of dephlogiſticated and inflammable air to be equal to that extricated by the burning of phoſphorus, we ſhall find, that the union of 120 grains of inflammable air with 576 grains of dephlogiſticated air, extricates 9265° of heat.

18. In the experiment on the deflagration of nitre with charcoal, by Meſſ. LAVOISIER and DE LA PLACE, an ounce of nitre and one third of an ounce of charcoal melted twelve ounces of ice. Suppoſing the ounce of nitre to have produced half an ounce of dephlogiſticated air, it ought to have conſumed 0,1507 ounces of charcoal, and ſhould have melted 14,773 ounces of ice; and I ſuppoſe it fell ſhort of its effect by the heat not being ſufficiently intenſe to decompoſe the nitre perfectly.

19. By the above gentlemen's experiment an ounce of charcoal required for its combuſtion 3,3167 ounces of dephlogiſticated air, and produced 3,6715 ounces of fixed air; therefore there was united to each ounce of air, when changed into fixed air, 61,5 grains of phlogiſton, and 3988° of heat were extracted. *It appears by theſe facts, that the union of phlogiſton, in different proportions, with dephlogiſticated air, does not extricate proportional quantities of heat.* For the addition of 61,5 grains produces 3988°. and the union of 120 grains produces 9265°. This difference may ariſe from a miſtake in ſuppoſing the ſpecific gravity of the inflammable air Dr. PRIESTLEY employed to have been only $\frac{1}{9,6}$ of that of dephlogiſticated air; for if it be ſuppoſed that its ſpecific gravity was a little more than $\frac{1}{8}$ of that of the dephlogiſticated air, then equal additions of phlogiſton would

have produced equal quantities of heat[*]: this matter ſhould therefore be put to the teſt of experiment, by deflagrating dephlogiſticated air with inflammable air of a known ſpecific gravity, or by finding how much dephlogiſticated air is neceſſary for the combuſtion of an ounce of ſulphur, the quantity of phlogiſton in which has been accurately determined by Mr. KIRWAN; or by finding the quantity of phlogiſton in phoſphorus, the quantity of dephlogiſticated air neceſſary for its decompoſition being known from Meſſ. LAVOISIER and DE LA PLACE's experiments.

On conſidering theſe latter gentlemen's experiments on the combuſtion of charcoal, a difficulty ariſes, to know what became of the remainder of the ounce of charcoal; for the dephlogiſticated air, in becoming fixed air, gained only the weight of 0,3548, or about $\frac{1}{3}$ of an ounce; about $\frac{2}{3}$ of an ounce are therefore unaccounted for. The weight of the aſhes of an ounce of charcoal is very inconſiderable; and, by ſome experiments of Dr. PRIESTLEY's, charcoal, when freed from fixed air, and other air which it imbibes from the atmoſphere, is almoſt wholly convertible into phlogiſton. The cauſe of this apparent loſs of matter, I doubt not, theſe gentlemen can explain ſatisfactorily, and very probably in ſuch a manner as will throw other lights on the ſubject.

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[*] Or it may ariſe from my being miſtaken, in ſuppoſing that the ſame quantity of heat is diſengaged by the union of dephlogiſticated air with phlogiſton, in the form of inflammable air, as is by its union with the phlogiſton of phoſphorus or ſulphur; and there appears to be ſome reaſon why there ſhould not; becauſe in theſe latter caſes the water, being united to the acids, cannot retain ſo much elementary heat as it can do when left in the form of pure water, which is the caſe when the inflammable air is uſed.

It is alſo worthy of enquiry, whether all the amazing quantity of heat let looſe in theſe experiments was contained in the dephlogiſticated air; or whether the greateſt portion of it was not contained in the phlogiſton or inflammable air. If it was all contained in the dephlogiſticated air, "*the general rule is not "fact, that elaſtic fluids are enlarged in their dimenſions in propor-"tion to the quantity of heat they contain*;" becauſe then, inflammable air, which is ten times the bulk of dephlogiſticated air, muſt be ſuppoſed to contain no heat at all; "and it is known, "from ſome experiments of my friend Dr. BLACK's, and ſome "of my own, that the ſteam of boiling water, whoſe latent "and ſenſible heat are only 1100°, reckoning from 60°, or tem-"perate, is more than twice the bulk of an equal weight of "dephlogiſticated air." It ſeems, however, reaſonable to ſuppoſe, that the greater quantity of heat ſhould be contained in the rarer fluid.

It may be alledged, that in proportion to the quantity of phlogiſton that is contained in any fluid, the quantity of heat is leſſened. But if we reaſon by analogy, the attraction of the particles of matter to one another in other caſes is increaſed by phlogiſton, and "bodies are thereby rendered ſpecifically "heavier;" and we know of no other ſubſtance beſides heat which can be ſuppoſed to ſeparate the particles of inflammable air, and to endow it with ſo very great an elaſtic power, and ſo ſmall a ſpecific gravity. On the other hand, if a great quantity of elementary heat be allowed to be contained in inflammable air, on account of its bulk, the ſame reaſoning cannot hold good in reſpect to the phlogiſton of phoſphorus, ſulphur, charcoal, &c. But all theſe ſubſtances contain other matters beſides phlogiſton and heat. The acids in the ſulphur and phoſphorus, and the alkali and earth in charcoal, may attract the phlogiſton ſo powerfully that the heat they contain may not be able to overcome the adheſion of their particles, until, by the effect of external heat, they are once removed to ſuch a diſtance from one another as to be out of the ſphere of that kind of attraction *.

If it be found to be a conſtant fact, that equal additions of phlogiſton to dephlogiſticated air do not extricate equal quantities of heat, that may afford the means of finding the quantities of heat contained in phlogiſton and dephlogiſticated air reſpectively, and ſolve the problem.

Many other ideas on theſe ſubjects preſent themſelves; but I am not bold enough to trouble you, or the public, with any ſpeculations, but ſuch as I think are ſupported by uncontroverted facts.

I muſt therefore bring this long letter to a concluſion, and leave to others the future proſecution of a ſubject which, however engaging, my neceſſary avocations prevent me from purſuing. I cannot however conclude, without acknowledging my obligations to Dr. PRIESTLEY, who has given me every information and aſſiſtance in his power, in the courſe of my enquiries, with that candour and liberality of ſentiment which diſtinguiſh his character.

I return you my thanks for the obliging attention you have paid to this hypotheſis; and remain, with much eſteem, &c.

JAMES WATT.

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* On the whole, this queſtion ſeems to involve ſo many difficulties, that it cannot be cleared up without many new experiments.

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